of Bruges (*c.* 1600), and although they must have been employed by scientific engineers, such as Brunel, the subject of Graphical Statics as known to the mathematician dates only from Maxwell's writings on the subject, and to Culmann's elaborate treatise in German; also to Colonel Sir George Clarke's exhaustive work.

The subject of Statics, which had come to a standstill, was revivified by the graphical methods now employed by every engineer and architect.

But as the subject is nothing unless employed practically by the draughtsman on the drawing board, it has not yet conquered the prejudices of the abstract mathematician, although many problems of allied descriptive geometry, required in the construction of inertia ellipses and curves (Part iii.), are well worthy of the attention of the pure geometer.

The present treatise is designed as an elementary textbook for the use of students of engineering; and the illustrations are drawn carefully to scale, representing each some real object.

The method of lettering, attributed to Bow, is now more appropriately assigned to Henrici; the author very rightly insists upon the fundamental importance of this lettering, in emphasising the reciprocity existing in the diagrams.

Incidentally the method of Graphical Statics emphasises the proper treatment of Statical problems, which is always to consider a system of balancing forces; and thus to banish the word Resultant from Statics unless employed to represent the force which if reversed will balance the remaining forces of the system. G.

A Naturalist on the Prowl. By Eha. Pp. 257. (London: W. Thacker and Co., 1894.)
From Spring to Fall. By "A Son of the Marshes." Edited by J. A. Owen. Pp. 239. (London: William Blackwood and Sons, 1894.)

THE author of "A Naturalist on the Prowl" knows how to write pleasantly on the natural history of the Indian jungle. There is not a dull page in his book. It is only rarely that we meet with a volume so full of interesting observations, and so free from stodginess. In "Eha's" company we travelled from the first to the last page, here admiring the keenness of his perception, there laughing at his humorous comments, and always made happy by his geniality. He does not "prowl" to kill, neither is he imbued with the spirit that induces many people to collect shells and postage-stamps as specimens; for though he recognises that "without a collection, a man's knowledge of natural history becomes nebulous, and his pursuit of it dilettante," he also knows that there is a possibility of a man degenerating into a mere collector, and ceasing to be a naturalist. Mr. R. A. Sterndale enriches the volume with eighty illustrations, mostly sketched from life.

The works of "A Son of the Marshes," on country life and scenery, are renowned for their simple beauty and sympathetic expression. Under the editorship of Mrs. Owen, the volume before us, like other books by the same author, is delightful reading.

Edible and Poisonous Mushrooms. By Dr. M. C. Cooke. (London: S.P.C.K., 1894.)

IT may be safely asserted that fewer kinds of fungi are used for food in Great Britain than in any other country in Europe. This is the more remarkable when we take into consideration the indebtedness of the present advanced state of Mycology to the researches of our countrymen, amongst whom may be mentioned Bolton, Sowerby, Badham, Berkeley, and Broome. The author of the work under consideration has also contributed very materially to a knowledge of edible kinds of fungi by various publications, and more especially in promoting annual fungus forays in various parts of the country. Poisonous fungi liable to be confounded with the numerous edible kinds are very few in number, and the majority of casualties, both at home and abroad, are caused by eating *Amanita phalloides*, a fungus very different in appearance from the common mushroom (*Agaricus campestris*), but which, probably from its neat and attractive appearance and size, appears to commend itself to unsuspecting persons, and being usually very abundant and widely distributed, is likely to be a continual source of danger until its characters and general appearance are more generally known.

Dr. Cooke very properly condemns the various rule-of-thumb methods for discriminating between edible and poisonous kinds of fungi, and shows that the essential characters of the various kinds must be thoroughly grasped, as being the only certain means of identification; and this method, with Dr. Cooke's book as a guide, should not prove a difficult task. The written descriptions of the various kinds, without being technical, are very clear and to the point, and the eighteen coloured plates are excellent. Finally, the best methods of cooking are given. The book is well printed, attractive externally, and very cheap.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

What are Acquired Characters?

FOR some while past, as we all know, a great contest has been raging as to whether acquired characters of an organism can or cannot be transmitted from one generation to another; and mighty authorities, on the one side, say that they can be; and great authorities, on the other, aver that they cannot be.

As a spectator of this contest, I have tried to understand it; and, in the first place, I have endeavoured to make out what is meant by the phrase "acquired characters"; or, in short, what is meant by the word "acquired," as used, in this connection, by Weismann, his friends, and his antagonists.

It is evident that the word is not used in its primary and natural signification; for, as on the theory of evolution (on which hypothesis the whole discussion proceeds), man has been evolved from an amœba or an ascidian, or some other early form, it follows that every character by which a man differs from this, his first progenitor has been acquired at some time between the two termini of the course of evolution, and, if the word were used in its ordinary sense, it would further follow that none of these characters could be transmitted by man to his offspring. This is manifestly untrue, for the issue of a woman is not simply an amœba. In fact, Weismann himself implies plainly that he does not use the word "acquired" in its ordinary signification, and asserts that its scientific value lies in its restricted use. ("Essays on Heredity," English translation, vol. i., first edition, p. 412.)

It becomes then very important to get at an accurate and workable definition of the word "acquired" for the purpose in hand; and such a definition must, I conceive, satisfy the following conditions:—(1) It must be such as to include all characters that are "acquired" within the restricted meaning of the word, and to exclude all characters that are not within the meaning; (2) it must be stated in physical, and not in metaphysical terms; (3) it must not be stated in terms derived from hereditability or the contrary, or in terms of any hypothesis or theory; (4) in order that it may be of use for scientific purposes, it must be stated in terms that admit of ascertainment and verification.

Of the importance of a clear definition of these words every one must, I think, be conscious; and if authority were required, we have that of Prof. Weismann himself. "I should wish to point out," he says, "that we ought above all to be clear as to what we really mean by the expression 'acquired character.'" ("Essays," vol. i. p. 169.)

Now, I do not profess to have read all that has come from the pen of Prof. Weismann, and still less the whole literature that has gathered round the controversy; but, after some search, I have hitherto failed to find any definition which has satisfied me; and, furthermore, I do not feel quite sure that all the advocates and opponents of Weismann use the phrase in one and the same meaning; and my object in writing this, is to ask for some assistance in finding the real and true definition of the phrase as used by both sides in this controversy.

Let me, to assist in the discussion, refer to some of the hints for a definition which I have been able to find.

At pp. 98–99 of vol. i. of the "Essays upon Heredity" (English translation, first edition), Prof. Weismann, having referred to "modifications which appear as the direct consequence of some alteration in the surroundings," and to the effect of a strange climate, says: "It is difficult to say whether the changed climate may not have first changed the germ, and if this were the case, the accumulation of effects through the action of heredity would present no difficulty. For instance, it is well known that increased nourishment not only causes a plant to grow more luxuriantly, but it alters the plant in some distinct way, and it would be wonderful indeed if the seeds were not also larger and better furnished with nutritive material. If the increased nourishment be repeated in the next generation, a still further increase in the size of the seed, in the luxuriance of the plant, and in all other changes which ensue, is at any rate conceivable, if it is not a necessity. But this would not be an instance of the transmission of acquired characters, but only the consequence of the direct influence upon the germ cells, and of better nourishment during growth."

This passage hints plainly at a definition of acquired characters to this effect. An acquired character is one produced by an external stimulus acting on the organism but not influencing the germ cells, whilst every character produced by an external stimulus acting on the organism and influencing the germ cells would not be acquired. But how are we to ascertain whether the germ cells—not, be it observed, the embryo, but the germ cells—have been influenced? Is there any chemical or microscopic means of answering this question? Is this influence a physical fact capable of ascertainment, and if so, how? It seems almost as if the presence or absence of an influence on the germ cells respectively, were inferred from the capacity or incapacity of transmission. But if so, I can hardly suppose that any one would suggest that any light can be got from such a definition, for this would be to proceed in a circle, and to reduce the statement that acquired characters cannot be transmitted to the following identical and useless proposition, viz. characters which by experiment are found not to be transmitted, and are therefore said not to affect the germ cells, are not capable of transmission, *i.e.* characters incapable of transmission are incapable of transmission.

At page 170 of the same volume occurs a passage which seems to suggest a slightly different definition of acquired characters. "I am also compelled to admit," says our author, "that it is conceivable that organisms may exert a modifying influence on their germ cells, and even that such a process is, to a certain extent, inevitable. The nutrition and growth of the individual must exercise some influence upon its germ cells." But a little afterwards, p. 171, there occurs another passage which, if I understand it aright, throws doubt on this conclusion; but this I will for the present neglect. In other passages, *e.g.* at p. 406, our author refers to direct influence of an external stimulus, such as climate, intending, I conceive, to draw the distinction between direct and indirect influence of an external stimulus on the germ plasma. These passages appear to me to suggest the following propositions with reference to acquired characters, viz.: (1) Every change produced by an influence of the organism on its own germ cells is not an acquired character; (2) an external stimulus may act on the organism, and the organism on the germ cells, and so produce a non-acquired character; (3) every other change produced in an organism by an external stimulus is an acquired character.

But, assuming these propositions to be true, do they admit of ascertainment by any appeal to physical facts? Do we know by any examination—physical, chemical, or experimental—what influence an organism produces on its own germ cells? If we do, then these propositions may be useful, and acquired characters will be a category of changes capable of scientific establishment by the appropriate means of inquiry; but if not, then they seem useless, except for the purpose of propounding an hypothesis or theory.

At p. 169 of the same volume of "Essays upon Heredity," I find Prof. Weismann saying: "An organism cannot acquire anything, unless it already possesses the predisposition to acquire it: acquired characters are therefore no more than local or sometimes general variations which arise under the stimulus provided by certain external influences"; and then he proceeds to illustrate this by saying that the so called "exercierknochen" or bony growth caused by the pressure of a weapon in drilling depends on the capacity of the bone to react on the stimulus. "Every acquired character is simply the reaction of the organism upon a certain stimulus."

It would not, I think, be just to consider that in the sentence last quoted, Prof. Weismann proposes a definition of an acquired character as being the reaction of the organism upon an external stimulus, because many passages in our author's writings, and certain well-known facts, seem to show that there are many reactions upon stimuli which result in heritable characters. Thus, at p. 406, he says: "Every one will agree with him (Delmer) that the periodical change of leaf in temperate climates has been produced in relation to the recurring alternation of summer and winter. This is certainly the case, and it cannot be doubted that this character has been fixed by heredity." Heat and cold are external stimuli, and here, if I understand rightly, they are credited with heritable changes in the organism.

Some passages in Prof. Weismann's Romanes lecture tend in the same direction. At pp. 10, 16, and 50 he deals with the case of geotropism, positive and negative; and, if I rightly follow our author, he alleges (1) that geotropism, or the habit of the plant to respond in some particular way to the force of gravity, was not an original character of plants; (2) that it arose, or, in popular parlance, was acquired when plants became attached to the ground; and (3) that it is inherited. Now if I apply to this what is, as I understand, taught by the Professor, I conclude that the physical characters which produce the habit are due to the concurrence of two things, viz. (*a*) the original predisposition and (*b*) an external stimulus, viz. gravity. From this it would seem to follow that if all "acquired characters" are reactions on external stimuli, yet some such reactions are not "acquired characters."

The more I look at the matter, the more I feel it impossible to suppose that all the reactions on external stimuli are "acquired characters." For when I consider the vast part played by air, food, heat, moisture, gravity, and light, all of which are external stimuli on the development of plant life, and as I gather from passages already cited from our author on the production of some qualities, such as size, colour, &c., which are familiarly known to be inherited, I feel it difficult to suppose that it can be thought that all responses to external stimuli are to be considered as incapable of transmission. If I try to arrive at a definition by drawing a distinction between the principal and the minor causes of a change in the organism, or by calling some things conditions and other causes, I succeed no better; for here I should be introducing metaphysical distinctions. There is, so far as I know, no physical or logical distinction between principal and minor causes, or between cause and conditions in the case of two or more constituent parts of a cause, each of which is necessary, and none of which is by itself sufficient.

But this line of thought carries me further. Prof. Weismann ("Essays," vol. i. p. 411) deals with the case of "spontaneous characters, such as extra fingers or toes, patches of grey hair, moles, &c.," which he says may be transmitted. But do we know (*i.e.* do men of science know) that no external stimulus has had anything to do with the production of, say, a mole? It is one thing not to know affirmatively that this is the case, and another thing to know that it is not the case. Is the distinction between characters which seem to be due to an external stimulus and characters which seem to be spontaneous, one which is the subject of accurate scientific knowledge? Seeing that we only know organisms when subject to stimuli, do we know what they would be or would produce without stimuli? Have we any scientific knowledge of the organic world as developed entirely *ab intra* and independently of any external influence, *i.e.* of what plants and animals would be without light, heat, food, air? If we have not, then the distinction relied on may be perfectly true, but is of no value for scientific reasoning at the present day.

Then it has occurred to me to inquire whether I can make a safe distinction between the two kinds of change by reference to the development of the embryo. If a mole were to be found on the arm of a child at birth, we should be more inclined to regard it as spontaneous, as due to no external cause, than if it appeared in mature life. The embryo in the case of a child is no doubt protected from many external stimuli; but surely in the case even of the placental mammalia it would not be safe to aver that the embryo is protected from all external stimuli. In the case of an insect, the greater part of its development takes place under an exposure to external influences as complete as that of the adult insect; in the case of reproduction by gemmæ of multicellular animals, the protection must, I suppose, often be small or nothing at all; and in the case of the reproduction of the lower plants from gemmæ or from protonema, or of the higher plants by buds or from suckers, the embryonic condition, if it can be spoken of at all, is, I suppose, hardly distinguished as regards the influence of external stimuli from any other part of the career of the organism; so that I find myself unable to reach clear ground for the distinction between spontaneous and non-spontaneous variations.

But there are passages, to one of which I have already referred, which seem to suggest that the definition should be framed by reference to a distinction between a direct and an indirect influence, and that the definition should run thus: an "acquired character" is a reaction of the organism upon the direct influence of a stimulus, leaving reactions upon indirect influences to be treated as non-acquired characters. If so, what is the precise meaning of the word "direct" as applied to the influence? Does it refer to the repetition of the stimulus—so that a single change of climate would be a direct, and a repeated change of climate an indirect influence? Or does it refer to the supposed difference between influences operating on the somatic part of the organism and those which the organism itself exerts on its germ plasma? If so, I ask how is this ascertainable as a physiological fact?

Another limitation on the proposition that the reactions of the organism on external stimuli are acquired characters, I find in the second volume of Weismann's "Essays on Heredity" (English translation, p. 14), where the author, having referred to the characters, such as shape and size of finger-nails, likenesses of features, bearing, gait, handwriting, which are handed down from parent to child, goes on to add: "Characters only acquired by the operation of external circumstances acting during the life of the individual cannot be transmitted." Now, handwriting must, I suppose, be conceived of as a thing dependent on external circumstances: it is influenced by the material on which, the fluid by which, the pen or style by means of which the act is performed; but here the external circumstances have operated during many generations; so that the passage seems to suggest these propositions, viz. the reactions of the organism on external stimuli operating during the life of a single individual are not hereditary; the reactions of the organism operating during the lives of two or more individuals are hereditary. But such is not, I suspect, really the meaning of the author; it would be inconsistent with what he says at p. 40 of the same volume, where he says that the supposed increase of the musical sense "in the course of generations" by the exercise of the art can only have occurred on the supposition "that these modifications of an organ which are due to the exercise during the individual life can be transmitted to offspring"—a supposition which Prof. Weismann says "a close examination does not allow us to admit."

Another limitation on the class of reactions upon external stimuli constituting "acquired characters" is suggested by what I conceive to be Prof. Weismann's latest utterance on the subject, in his work "Das Keimplasma" (Jena, 1892). At p. 514 I find him saying—"By the term acquired characters I understand those which do not exist originally in the germ as tendencies, but first arise through peculiar influences which operate upon the body or particular parts of it. They are the reactions of these parts upon some external influences lying beyond the necessary conditions of development."[1] In the first of these sentences it seems to me that the Professor is not so much offering a definition as announcing a theory; for except by the inquiry whether the character is or is not heritable, I suppose that there is no means of ascertaining whether or not a change in the organism is due to a tendency in the germ.

But the second sentence seems to suggest a definition of "acquired characters" free from many of the difficulties we have hitherto encountered. They do not include all reactions of the organism on external stimuli, but only such as lie beyond the necessary conditions of development. Everything then turns on the meaning of "necessary conditions of development." What are necessary conditions of development? Let us take a tree which has put forth its leaves, its flowers, its seeds, in usual fashion, but which, having lost a limb by the saw of the gardener, and has thrown out around the wound that growth of new wood and bark with which we are familiar. The air, the sun, the soil are all external influences, and all necessary conditions of the development *which has actually occurred*, and so was the saw of the gardener. If we take the development which has actually occurred, every condition which led up to it was necessary, and each was as necessary as the other. Take the case narrated by Sir James Paget, of a fir tree which for a hundred and fifty years threw out successive annular growths over the part of its trunk from which a large piece of bark had been stripped off. ("Address to the Pathological Section of the British Medical Association," 1880, p. 15.) Were these rings part of its development, or were they not? If they were, the knife or saw was one of its necessary conditions. Most persons will reply that the saw was no necessary condition of the development of the fir tree, and that those only were necessary conditions without which the fir tree could not have lived.

If we consider what are the necessary conditions for the development of the seed of the fir tree, we can probably ascertain them; but the doctrine of evolution and the doctrine of the non-heritability of acquired characters will carry us back further, viz. to the primordial organism, and we must ask what were the necessary conditions for its development.

If this organism were supposed to have in itself a pre-existing law according to which it sought development, a contingent destiny inherent in its nature, then I can well understand how the conditions which satisfy that contingency, the circumstances which allow of that development might be said to be necessary to it. But if the organism have no such law and no such destiny, but instead thereof has only a capacity to vary in every possible direction, and if all the course of its actual variation be due to external circumstances operating by means of natural selection, then it seems to me that no one external thing can be said to be more essential to its development than another. The germ might have found itself in a different soil, in a different climate, exposed to different air, and then the development of the germ would have been different. There was no *à priori* necessity that it should be exposed to the particular conditions to which it was, in fact, exposed, or to any other particular conditions. If, therefore, we start from the actual development of an organism, and look back on the past, all the conditions which have led to its existing state are necessary; if we start from the germ prior to all development, and look to the future, then no given condition can be said to be necessary to its development, but all are contingent.

It is suggested that no influence lying beyond the necessary conditions of development can at any time have produced any heritable character, however long may have been the course of development. It seems, therefore, that in order to ascertain what conditions are necessary to development, we must go back to the amœba or ascidian or other primæval parent of our race, and we must conclude that those characters only will be transmissible by the human parent, which were reactions on the necessary conditions of the development of the primæval ancestor. Have we any scientific means of ascertaining what these conditions were, and so of ascertaining what characters are now heritable? Nay, if we adopt the evolutionary hypothesis, and believe that at least all existing animal organisms have sprung from a single parent, do not the diversities of the existing forms show that no one set of external circumstances were necessary conditions of development, but that the conditions consistent with development were infinite, or all but infinite, in number?

A further difficulty arises in my mind from a passage on the next page (p. 515), where I find our author mentioning wounds and mutilations as constituting one category of acquired characters. It is difficult to reconcile this with the statement of the Professor, in several passages, that acquired characters are reactions of the organism; for surely a wound is not a reaction of the organism, whilst the growth of the organism consequent on the wound—*e.g.* the growth of new wood and bark round the stump of a branch sawn off, is a reaction of the organism on the action of an external influence. It seems, therefore, fair to suppose that when our author speaks of a mutiation as an acquired character, he means the growth of the organism consequent upon the mutilation. But if so, a difficulty appears to arise; for the tendency to repair a wound is heritable, and therefore it seems difficult to suppose that the Professor would treat of it as not heritable. It may be said that the parent has the actuality of the repair, the child only the possibility of repairing; but this is, I suppose, all that can ever be expected of inheritance as applied to contingent reactions—*i.e.* reactions which only arise under peculiar circumstances. There can, I suppose, be no more characteristic heritable reaction than that of the pollen on the ovule, and of the ovule on the pollen; these reactions have taken place in the parent plant, but in the offspring they are originally potentialities, tendencies, contingencies, and they are converted into facts in the event, and in the event only, of the meeting of the pollen and the ovule.

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[1] It may be well to give the passage in the original: "Unter erworbenen Eigenschaften verstehe ich solche, welche nicht als Anlagen schon in Keim vorhanden sind, sondern erst durch besondere Einwirkungen, die den Körper oder einzelne Theile desselben treffen, enstehen. Sie sind die Reactionen dieser Theile auf irgend welche, ausserhalb der nothwendigen Entwickelungsbedingungen liegenden äusseren Einwirkungen."

Or take, again, the secretion of the gastric juice in response to the presence of food in the stomach. The parent has taken food, and the reaction has taken place; but the infant inherits, I suppose, the capacity to secrete and not the counterpart of the actual secretion of the parent.

It would seem that there is no essential difference between these three cases. The parent transmits the power to repair a wound, but not the actual reparation of a wound: it transmits the power of fertilisation, but not the fertilised ovules: it transmits the power to digest, but not the already secreted gastric juice.

The emphasis which Weismann has laid on the case of wounds and mutilations would suggest that his doctrine might be thus paraphrased: when an organism is endowed with a capacity, or, to use his word, a predisposition, to react in response to given stimuli, and has so reacted—then what the organism transmits to its progeny is the capacity or predisposition, and not the actual result of the reaction.

It is impossible to doubt that some characters of an organism are hereditary; that others are not, and that the ascertainment of the dividing line between the two classes is of the highest moment to the study of biology; and to Weismann we owe a debt of gratitude for having called pointed attention to this matter. I have at times been tempted to wish that men of science had applied themselves to ascertain the two categories of characters, and then by a careful induction had proceeded to learn the law of heredity without regard to any hypothesis or theory—without reference to germ or soma. But this is not the course which in fact has been taken; and therefore it seems highly necessary to inquire what is the precise meaning of the terms of the proposition affirmed by the one side, and denied by the other.

I conclude this, I fear, too lengthy paper with two questions: (1) Are the conditions which I have suggested as essential to a good definition correct ones; if not, in what are they erroneous? (2) What is the true definition of the words "acquired characters" in the present controversy which satisfies these conditions?

EDW. FRY.

Discontinuous Motion.

THE old theory of the motion of solid bodies through a frictionless liquid supposed that the liquid flowed according to the electrical law of flow. This theory was found to be unsatisfactory, because it makes the pressure negative when the velocity of the solid exceeds a certain critical value.

The theory of discontinuous motion removes the above objection, but is open to others of a different kind. Assuming for the sake of argument that the two theories give correct results when the velocity of the moving solid is respectively less and greater than the critical value, the theory of discontinuous motion ought to be capable of explaining the transition from one kind of motion to the other, and how and why it is possible for a vortex sheet to be called into existence when the critical value of the velocity is exceeded.

Although vortex sheets and other motions involving molecular rotation cannot be generated in a frictionless liquid by a conservative system of forces or by operations performed on the boundary, yet it is easy enough to produce such motions by ordinary mechanical agencies. If a mixture of ice and water be stirred up and the ice allowed to melt, the liquid will acquire molecular rotation owing to the presence of the particles of melted ice, even though it is absolutely devoid of viscosity. So also if liquid at rest were separated by an indefinitely thin horizontal plate, and the upper liquid were set in motion with horizontal velocity V and the plate were removed, the surface of separation would be a vortex sheet. But the production of motions of this kind requires methods of a somewhat artificial character, and it is difficult to see how they could be set up by a solid whose velocity is allowed to increase gradually from zero to some magnitude greater than the critical value. In fact, I entertain very little doubt that the final motion of the liquid would be quite different from what the theory of discontinuous motion would indicate.

There is, however, a further point, for there are strong grounds for believing that vortex sheets are unstable. No general proof of this proposition appears as yet to have been given, but in every case that has been examined the theorem has been found to be true, (i.) when the liquids on either side of the sheet are identical, (ii.) when the densities of the two liquids are different, but no bodily forces such as gravity and the like are in action. If, therefore, steady discontinuous motion existed at any particular instant, the probabilities are that the motion would be unstable, and the region of dead water in the rear of the moving solid would break up and be changed into a region of turbulent motion. The pressure in the rear of the solid due to this turbulent motion would be different from that of the dead water, and it is therefore not surprising that the theory of discontinuous motion should furnish results which do not agree very well with experiment.

We must also recollect that a frictionless liquid is an ideal substance which does not exist in nature. All fluids are more or less viscous; and it is just at the point where the pressure would otherwise tend to vanish and change sign that we should anticipate the effect of viscosity would appear, and prevent this state of things from taking place; and I believe that many of the difficulties which have arisen in connection with this subject are due to the fact that the effect of viscosity has been overlooked. A vortex sheet cannot exist in a viscous liquid; and if by any artificial means one were produced, it would immediately disappear, and molecular rotation would be propagated into the surrounding liquid. On the other hand, in a viscous liquid, molecular rotation requires no artificial means for its production; for a viscous liquid cannot move without molecular rotation, except in the single case in which the liquid moves like a rigid body having a motion of translation alone. In all other cases, if irrotational motion existed at any particular instant, the motion would immediately cease to be so, and molecular rotation would instantaneously be generated.

Unfortunately the equations of motion of a viscous liquid are so intractable that very little progress has been made in applying them to the solution of hydrodynamical problems. By means of Oberbeck's solution (*Borchardt's Journal*, vol. lxxx.) for the steady motion of translation of an ellipsoid in a viscous liquid, it can be shown that the above difficulties do not arise when viscosity is taken into account; but since the integration of the equations of motion proceeds upon the assumption that the squares and products of the velocities may be neglected, the solution is inapplicable except in the case of slow motions like those produced by the small oscillations of pendulums. The solution gives no information as to what will happen when a disc is moving through a liquid with a velocity of several feet per second.

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Capacity for Heat.

IN the course of some writing upon which I am now engaged, I have constantly to refer to the *capacity for heat of certain substances as compared with the capacity for heat of an equal volume of water*.

The phrase given in italics is a most clumsy one, but I know not how (accurately) to convey the same idea in a shorter way. "*Capacity for heat of unit volume*" has been suggested; but I think that a little reflection will show that it does not express accurately the exact meaning.

Specific Heat × *Specific Gravity* gives the numerical value required, but cannot be regarded as a definition. There can be no doubt but that a concise expression is wanted. In calorimetry it is often of greater importance to the experimenter to onsider the capacity for heat of volumes rather than the capacity